August 24, 1953

Dear Phil:

Just got back from SanFRnancisco. We had to hurry back to take care of miscellaneous business here, and moving to a new house in a week. It was probably a mistake to drive out, as we couldn't take enough time to make it any sort of vacation. All in all, it will be probably 3-4 weeks before the lab. here will be back in decent operation.

I will assume that you will have taken any action you wish to with regard to reprints of our J. Immunol. paper. Did I mention that the galley seemed to indicate the September issue, though this seems all too soon.

I am willing to agree that the cross-reactions of kunzendorf 302-50 are insignificant.

Are you planning to do anything in particular about publishing the serology of 1043-G2? I would think the characterization of S. gallinarum would be of some particular interest. If this is your thought too, I think I should try to effect some other transductions of gallinarum to some other serotypes, to check whether gm always shows up regardless of the constitution of the recipient, as should be the case.

I still intend to adhere to my promise to review our correspondence and tidy up some of the loose ends at the back of my own mind. I hope we can still get together on checking Iseki's E_1-E_2 business. If you do now have some IV XII serum, free of H-agglutinins, it might be useful for continuation of some of the abortus-equi problems.

I have to admit that CDC-2034/53 which you returned to me, having received it from me as "TM2 ph2" is a monophasic 1,2:—, as is the stock from which I subcultured it. This seems to be the story. You may remember my previous complaints that the ph2 of TM2 agglutinated in i serum rather more strongly than the incidence of phase variation would suggest. Anyhow, I picked a lot of colonies to find one that did not agglutinate in i, and put this away as "TM2 ph2". It seems to be actually a monophasic mutant of TM2, and therefore of great interest. I have since gotten out again the typically diphasic ph1 and ph2 of TM2, and will send you the same after we've collected ourselves. For future reference, the monophasic=2034/53=SW-1061.

Had I mentioned before the name Aleck Bernstein— he's expected to arrive in a few weeks as a "graduate student#, and candidate for Ph.D. in genetics. Actually, he holds a British M.B. and M.B. as well as a Dipl. Bact. from the London School of Hygiene. Although I expect he will be working here mainly on Salmonella (e.g. transduction in other groups) he has also had a good deal of experience with E. coli 055 and 0111 in England. Esther has had some troubles with some of the cultures more recently sent up— one didn't grow out at all, and another was streptomycin-resistant discrepantly with our notes on an earlier subculture. But we thought to wait to go over the problem with Bernstein, and plan our program more carefully, before begging for any other material or trying to wring any conclusions from present details. We have not yet gotten to the point of any serological testing of recombinants. Bernstein is also bringing over a fair amount of his own material. As you might have guessed, we got together through Clive Spicer. Spike writes that he has still had no success in transduction of somatic antigens between groups B and D. He plans to investigate the "transduction" of I which claims to happen (and I doubt) in some paratyphi A experiments,(e.g. Kauffmann's).

Sincerely,

Joshua Lederberg